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Analyzing the Reinforcement Process at the Human Level: Can Application and Behavioristic Interpretation Replace Laboratory Research?

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Critics have questioned the value of human operant conditioning experiments in the study of fundamental processes of reinforcement. Contradictory results from human and animal experiments have been attributed to the complex social and verbal history of the human subject. On these grounds, it has been contended that procedures that mimic those conventionally used with animal subjects represent a "poor analytic preparation" for the explication of reinforcement principles. In defending the use of conventional operant methods for human research, we make three points: (a) Historical variables play a critical role in research on processes of reinforcement, regardless of whether the subjects are humans or animals. (b) Techniques are available for detecting, analyzing, and counteracting such historical and extra-experimental influences; these include long-term observations, steady state designs, and, when variables are not amenable to direct control (e.g., age, gender, species), selection of subjects with common characteristics. (c) Other forms of evidence that might be used to validate conditioning principles—applied behavior analysis and behavioristic interpretation—have inherent limitations and cannot substitute for experimental analysis. We conclude that human operant conditioning experiments are essential for the analysis of the reinforcement process at the human level, but caution that their value depends on the extent to which the traditional methods of the experimental analysis of behavior are properly applied.

Key words: human operant research, reinforcement principles, behavioral history, individual differences, steady-state methods, applied behavior analysis, behavioristic interpretation

Can principles of operant conditioning, based as they are on findings from the animal[1] laboratory, contribute to a comprehensive account of human behavior? Skinner long ago answered in the affirmative, and he provided an initial outline in *Science and Human Behavior* (Skinner, 1953) and in *Verbal Behavior* (Skinner, 1957). Because the account included no new systematic data from humans, only facts "well known to every educated person," Skinner described his effort as "an exercise in interpretation rather than a quantitative extrapolation of rigorous experimental results" (Skinner, 1957, p. 11). Presumably, more direct empirical tests would follow, and, eventually, a more precise and sophisticated understanding of human behavior would emerge.

Yet now, decades later, the question remains far from settled, and Skinner's optimism has been met by skepticism. Although Skinner's early critics came from outside the field of behavior analysis (e.g., Brewer, 1974; Chomsky, 1959;

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[1] In this article we will use the term "animal" to refer to nonhuman subjects. Although some writers have objected to this usage (e.g., Poling, 1984), it is well-established in the scientific community and has become an integral part of the vocabulary, as evidenced by the journal titled *Animal Learning and Behavior* and such phrases as "animal welfare," "animal sciences," and "Animal Care and Use Committee."

Spielberger & DeNike, 1966), they have been joined by insiders who also question the relevance of conditioning principles to the analysis of human behavior (e.g., Davey, 1988; Lowe, 1979, 1983; Schwartz & Lacy 1988; Wearden, 1988). Rebuttal of such criticism requires evidence of progress toward an operant account, but there appears to be a lack of consensus among behavior analysts about what constitutes such evidence.

Efforts to advance an operant account have taken several forms (Baron & Perone, 1982). The first follows the lines of Skinner's (1953, 1957) "exercises in interpretation." The aim is to identify instances of naturally occurring human behaviors in which the relations among stimuli, responses, and reinforcers appear to parallel those studied under the controlled conditions of the animal laboratory. The second approach applies procedures derived from the laboratory-based principles to deal with human problems. The success of the application then is taken as support for the validity and generality of the principles. The third approach is the experimental analysis of human behavior within the laboratory. The goals are to arrange direct tests of the interspecies generality of the animal-based principles and to isolate the contribution of these principles to more complex processes not easily discerned in animals, such as those involved in verbal and social behavior (Hake, 1982).

Behavior analysts of an interpretive bent can offer ingenious accounts of such fascinating topics as talking, listening, knowing, thinking, and creating. Applied behavior analysts can point to successful interventions in clinics, institutions, schools, and business organizations. Unfortunately, these achievements are not matched in the laboratory, where discrepancies in the performances of human and animal subjects—particularly in experiments on schedules of reinforcement—raise questions about the role of operant conditioning in human behavior (cf. Bandura, 1977; Brewer, 1974; Lowe, 1979; Wearden, 1988). Thus, despite apparent successes in the realms of interpretation and application, apparent failures in the laboratory have provoked somewhat of a crisis.

The response of the scientific community to this state of affairs undoubtedly will influence the future development of behavioral psychology. Some writers have offered the possibility that there are qualitative discontinuities in the principles governing human and animal behavior (Brewer, 1974; Hayes, 1987a, 1987b; Lowe, 1979). If so, then human and animal research must proceed independently, and there may be little overlap in the conceptions of behavior that emerge in the two domains. We have taken issue with this point of view elsewhere (Baron & Galizio, 1983; Baron & Perone, 1982; Perone, 1985; Perone, Galizio, & Baron, 1988).

There has been another, quite different, response to the discrepant results. The proposition is that a unified set of basic principles indeed does underlie both human and animal behavior, but, for various reasons, humans are not suitable subjects in experimental investigations of these principles (Dinsmoor, 1983; Michael, 1987; Shull et al., 1989). The purpose of the present article is to consider this second view in more detail.

THE SUITABILITY OF HUMAN SUBJECTS FOR BASIC RESEARCH

Although laboratory experiments with humans are the norm in psychology as a whole, they are rare in the study of operant conditioning (Buskist & Miller, 1982). What accounts for this disparity? It seems obligatory to point to special problems and procedures when humans are the subjects of operant conditioning experiments, but the obstacles are not insurmountable, nor are they necessarily higher than those faced by researchers in the animal laboratory (Baron & Perone, 1982; for a recent discussion of methodological aspects of human research, see Bernstein, 1988; Galizio & Buskist, 1988; Morris, Johnson, Todd, & Higgins, 1988; Perone, 1988; Pilgrim & Johnston, 1988). A more forbidding obstacle is the pessimistic view expressed by some behavior analysts about the contribution that research with humans can make to the analysis of behavior. Dinsmoor (1983) put the matter this way:

> I do not consider the human species a suitable one for the investigation of fundamental behavioral processes. This is like using some freshly mined, unrefined substance in a chemistry experiment. Human subjects bring with them substantial pre-experimental histories which frequently contaminate or cover up the underlying patterns of behavior. Moreover, human subjects have typically grown up in a social environment very similar to that of their experimenters, and they tend to conform in their already learned patterns of behavior to the expectations induced by that same culture in their experimenters. (p. 719)

Thus, the complex social repertoire brought by human subjects to the laboratory is seen as getting in the way of basic processes, perhaps by insulating the subject from the experimental contingencies or by disturbing the operation of critical variables once the contingencies are felt. In Dinsmoor's view, then, it seems that basic research on human operant behavior is misguided.

Although Dinsmoor's position may be extreme, others (e.g., Michael, 1987) have expressed similar doubts about the suitability of human subjects for basic operant research. The present article was prompted by Shull et al.'s (1989) review in this journal of Davey and Cullen's (1988) book on human operant conditioning. Unlike Dinsmoor, these reviewers affirmed the necessity and importance of laboratory research with humans. They did, however, take issue with such research when the goal is to explicate basic processes of reinforcement, in particular when the methods mimic those commonly used with animal subjects—a practice they characterize as the construction of a "human Skinner Box:"[2]

> One might wonder, then, exactly what purposes are served by studying the performances of humans on schedules of point production in tightly controlled laboratory arrangements. . . . If the purpose is to study the strengthening effects of reinforcement, the "human Skinner Box" might be an extremely poor analytic preparation. The combination of variables, given the elaborate repertoires and social/verbal histories, might simply be too complex to permit straightforward relationships to emerge. (pp. 72–73)

Shull et al.'s position is more moderate than Dinsmoor's in that they do not object to human operant research in general (indeed, the review indicates that they are strong advocates), nor do they reject the approach out of hand:

> Perhaps the "human Skinner Box" is a highly suitable preparation for studying complex forms of stimulus control (Sidman, 1986) and instructional control (e.g., Catania, Matthews, & Shimoff, 1982), even if it is not well suited for studying the strengthening effects of reinforcement. But perhaps there are better preparations for studying these important phenomena. (p. 73)

The objection, therefore, is to the study of fundamental principles of reinforcement by analyzing human performances within controlled environments when there are simple contingencies between brief, discrete responses and consequences—for example, contingencies between button-pressing and points or small amounts of money. These procedures, however, are the very ones used over the years in the laboratory study of human operant conditioning, beginning with the early studies of Azrin (1958), Holland (1958), Laties and Weiss (1960), Weiner (1962), and Baron and Kaufman (1966), and continuing to the present (e.g., Baron & Galizio, 1976; Baron & Journey, 1989; Perone & Baron, 1980).

A key issue—one not fully addressed in Shull et al.'s review—concerns the exact features of the method to which exception is being taken. Given the traditions and values of behavior analysis, it seems unlikely that the objection is to procedures intended to do no more than improve control. The obvious reason for using operant chambers with either animals or humans is to isolate the subject from unwanted influences. Within this controlled environment, special equipment then is used to program contingencies and record responses, allowing precise manipulation and measurement of the variables under investigation. But

[2] B. F. Skinner wrote, concerning "the apparatus known as the 'Skinner box,' " that this is "an expression which I have never used and which my friends accept as *verboten*" (Skinner, 1983, p. 164). We use the term "human Skinner Box" reluctantly.

as Bachrach (1981) noted, other critics have objected to experimental research precisely on these grounds. As alternatives to experimental methods (sometimes considered "dehumanizing" for the human subject), procedures are advocated whose distinguishing feature is the absence of control, for example, case studies, solicitation of verbal reports, and clinical observations in general.

In line with Dinsmoor's comment, Shull et al. focused on the special history of the human subject and the ways in which such a history may interact with reinforcement variables manipulated within the experiment. "Humans enter our experiments with complex repertoires, verbal and nonverbal, that differ in many ways from those of our nonhuman animal subjects. Who would doubt that these differences can matter?" (Shull et al., 1989, p. 72). But not every difference makes a difference, and we would encourage the view that questions about these differences are best approached empirically. Indeed, this was the theme of our chapter in the reviewed book (Perone et al., 1988). We asked whether the literature on human and animal responses to reinforcement schedules indicates a need for qualitatively different approaches. Although we acknowledged some puzzling discrepancies, our assessment was that the data do not support calls to abandon the traditional methods of the experimental analysis of behavior. This conclusion was based on three sets of observations and arguments:

1. *Procedures.* Research within the animal laboratory is characterized by standard and well-known procedures; the goal is to allow careful observations of steady-state performances after long-term exposure to the schedules. The literature on human operant behavior, by comparison, is replete with diverse methodologies; the objectives of a steady-state analysis often are not met because of too-brief exposure to the schedules.

2. *Inconsistencies.* The animal as well as the human literature contains numerous unanswered questions about essential variables and processes. This state of affairs does not provide a sound basis for concluding that human schedule performances are fundamentally different.

3. *Verbal behavior.* It remains to be established that a new set of principles is needed to account for the strong control exerted by verbal processes (instructions, rules, and the like) on reactions to reinforcement schedules. Results from the animal laboratory (e.g., studies of the effects of schedule history and stimulus control on current performances) suggest a common analysis in terms of the competition between contemporary and historical variables for the control of behavior.

HISTORICAL FACTORS IN HUMAN AND ANIMAL BEHAVIOR

Shull et al.'s (1989) remarks imply that historical variables play different roles in human and animal research. For the animal subject, laboratory procedures are regarded as excluding (or at least minimizing) the organism's history prior to the experiment, thus allowing undisturbed study of the experimental variables. The human subject's history, by comparison, is seen as overriding the influences of the experimental procedures. It is hardly our intention to downplay the complicated contribution that historical variables can make to human performances. Actually, Shull et al.'s review echoes our earlier discussion of "the place of the human subject in the operant laboratory" (Baron & Perone, 1982) where we acknowledged the formidable methodological problems that confront the investigator of human behavior. Aside from the diverse histories that human subjects bring to the laboratory, additional complications ensue from other extra-experimental factors including uncontrolled experiences between laboratory sessions and variations in such personal characteristics as age, gender, or educational background.

Unlike Shull et al., however, we were optimistic that remedies could be found from among the research designs and procedures of the animal laboratory, the methods at the very foundation of the experimental analysis of free-operant behavior (Ferster, 1953). An essential feature of these methods is their focus on the "steady state," a point of equilibrium in the continuous reciprocal interaction between behavior and the variables that influence it (Sidman, 1960). Attainment of steady states requires intensive study of the individual organism over a prolonged series of experimental sessions. The experimenter's ability to produce stable performances, as a consequence, indicates that relevant variables have been identified and brought under control, thus allowing observations of reliable functions between independent and dependent variables. Although steady-state methods have been the mainstay of the experimental analysis of animal behavior (Perone, in press), they appear to be considered more an option than a requirement in research with humans. Given this state of affairs, the conclusion we reached in the chapter of the reviewed book may bear repeating: In the absence of concerted efforts for long-term study of human performance as steady-states, it seems premature to conclude that humans are not suitable subjects in the basic experimental analysis of behavior.

Shull et al. (1989) stressed that historical variables play confounding roles in experiments with human subjects. The counterpart to this argument is that historical variables need not be a serious source of concern when the subjects are animals. Although this may be the conventional wisdom, a close look at the literature uncovers considerable evidence that parallel concerns are in order. This is hardly the place to attempt a systematic review of animal experiments that have investigated the role of historical factors (for a general review, see Denny & Ratner, 1970; for more recent discussions by behavior analysts, see Barrett, 1986; Wanchisen, 1990; Wanchisen, Tatham, & Mooney, 1989). Perhaps we can make our point by referring briefly to an issue in the area of attention and stimulus control.

A recurring problem for operant researchers has been the idiosyncratic stimulus preferences sometimes shown by experimental subjects. A classic example is Reynolds' (1961) experiment in which pigeons were trained to peck a red triangle but not a green circle. Subsequent tests with elements of these compound stimuli showed that responding had come under the control of either the form or the color of the stimulus, but not both (one bird pecked an achromatic triangle but not the color red; the other pecked the color red but not the triangle). This finding—that only some elements of a compound assume control—is, of course, important. Equally significant, however, is that the two birds unaccountably attended to different elements of the compound, presumably because of differences in their pre-experimental histories. In this respect, then, their behavior was poorly controlled by the experimental procedures.

Without further information, one can only speculate about the sorts of histories that led Reynolds' pigeons to attend to different stimulus elements, but a subsequent experiment by Thomas (1969) provides a clue. Thomas's research was undertaken to clarify Jenkins and Harrison's (1960) surprising finding about auditory generalization gradients. Flat gradients resulted from training with a single pure tone; decremental gradients, of the sort obtained after single-stimulus training with visual stimuli, appeared only after discrimination training with tones. Thomas reasoned that an important difference between visual and auditory stimuli is that laboratory pigeons are likely to have severely limited experience with pure tones. When he provided such a history in his research (by sounding the training tone in the housing area for 100 days before the experiment proper), orderly decremental gradients resulted.

Like the human subject, the laboratory animal devotes only a fraction of its time to actual participation in experiments. There is considerable evidence that the environments encountered by the animal outside the experiment can influence a range of behavioral processes. Consider, for example, the vast literature on enriched environments. In an experiment that led to many others, Hebb (1949) compared the maze learning ability of rats that had been raised as pets by his daughters with those raised in laboratory cages; the pet rats were superior. In this vein, Christie (1951) proposed that different outcomes and theories from Spence's laboratory and Tolman's laboratory may have been due to the richer housing environments provided by Tolman's group. Since then, there have been numerous investigations showing that housing conditions can have important behavioral effects. Research in this area is not often mentioned in connection with research on operant conditioning. The results may be relevant, however, particularly in the light of Hineline's (1986) suggestion that, in the interests of humane care, researchers should consider enriching the housing environments of laboratory animals. In this connection, one of us found that rats housed socially in large pens explored more, were less timid, and weighed less than animals housed individually (Menich & Baron, 1984). It would not be remarkable if such changes interacted with the influences exerted by operant conditioning variables. Operant researchers should proceed with caution, therefore, if they plan to introduce changes in the way they house their experimental subjects.

In considering the potential contribution of an animal's history to experimental outcomes, it is well to remember that control of historical variables cannot, in and of itself, reveal the interactive effects of the variables. Use of subjects with similar histories (e.g., animals reared in a common environment) has the advantage of increasing the power of the experiment by reducing the behavioral differences that accompany different histories. But this practice has the disadvantage of reducing the generality of the findings insofar as the outcomes depend on the level of the variables held constant. If all the pigeons in an experiment have been denied prior experience with pure tones, then conclusions about auditory generalization will be restricted to subjects with such histories. Similarly, if all the rats in an experiment have been raised in isolation and are overweight as a consequence, conclusions from experiments involving a percentage reduction in body weight may well differ from results with group-reared animals.

What does all this tell us about the use of human subjects in the analysis of basic behavioral processes? We have argued that unforeseen historical and other extra-experimental factors can play confounding roles in research with humans and animals alike. Curiously, this parallel in the determinants of human and animal behavior is not matched by parallel methodological directives. Thus, while concern about extra-experimental factors led Shull et al. (1989) to oppose the use of traditional methods with humans, they did not oppose using the methods with animals. We do not see the justification for such asymmetry.

In fact, conventional operant methods have proven to be of continuing value to the analysis of basic processes—in humans as well as in animals—despite the complicating effects of history and other extra-experimental factors. In a review of research practices with humans, Bernstein (1988) noted that a subject's initial reaction to an experimental condition may differ substantially from the steady-state performances observed after long-term exposure. When too few sessions are conducted, transitory phenomena arising from outside factors may be mistaken for experimental effects. The problem is especially acute with adult humans whose extensive verbal and social histories make them highly susceptible to control by instructions, rules, and the like, rather than by the contingencies imposed within the experimental environment. The antidote is to conduct long-term experiments that provide significant exposure to the experimental variables and allow them to compete with the verbal control already present. Otherwise, Bernstein argued, "we may overestimate the impact of instructions and underestimate the importance of environmental variables such as contingencies" (Bernstein, 1988, p. 55).

There can be no guarantees, however, that steady-state procedures will effectively counteract the influences of extra-experimental variables. In the case of the adult human subject, in particular, the experimental variables must compete with the well-established behavioral repertoires that the individual brings to the laboratory. Nevertheless, steady-state methods provide the best hope for detecting and analyzing interactions between historical and contemporary influences. Indeed, much of what is known about the effects of reinforcement histories on human schedule performances is the product of these methods.

An apt illustration comes from the seminal work of Weiner (1964, 1969) which showed that an experimental history with differential-reinforcement-of-low-rate schedules affected later responding on fixed-interval schedules, not only in terms of rate and pattern but also in terms of sensitivity to parametric manipulations of the interval (for further accounts of this and related research, see Weiner, 1983). Weiner's research is noteworthy in several respects: First, it was designed specifically to address the possibility that uncontrolled differences in pre-experimental histories may contribute to intersubject variability on free-operant schedules. Second, it showed that intersubject variability could be reduced in spite of these histories by providing new histories within the experiment itself. Third, it involved adult human subjects, a button-pressing response, and contingencies of point production—the very preparation that has been criticized as unsuitable for basic research on fundamental processes of reinforcement. Fourth, it set the standard for more recent studies of history effects in animals (Barrett, 1977; Egli & Thompson, 1989; Urbain, Poling, Millam, & Thompson, 1978; Wanchisen et al., 1989). In our view, Weiner's operant conditioning experiments with humans—research that relied heavily on methods borrowed from the animal laboratory—has provided much of the impetus for contemporary work that seeks to control history effects and thereby bring them within reach of an experimental analysis.

It is important to recognize that certain variables deemed important on the human level cannot be manipulated within the framework of laboratory experiments. We are referring to a diverse list of potential influences which includes the individual's age, gender, socioeconomic status, intellectual ability, educational level, personality, and the like. Here, too, there are lessons to be learned from animal research. In research on developmental processes or comparative questions, for example, it is not possible to manipulate the animal's age or sex or species. A different form of control is available through selection, however, in which case the behavior of animals selected in terms of their different ages, sexes, or species can be compared. And when such factors are not of immediate interest, it is routine to hold them constant to simplify analysis of the experimental variables. An important remedy, therefore, for the individual differences that human subjects may bring to the laboratory is to select subjects carefully in terms of characteristics that may be related to the variables under investigation. Our impression from published research is that more care could be exerted in this regard (see Morris et al., 1988, for further discussion of this issue).

IMPLICATIONS FOR AN OPERANT ACCOUNT OF HUMAN BEHAVIOR

The unavoidable question is this: If research fails to demonstrate the role of operant principles in human behavior under the relatively simple, highly controlled circumstances of the laboratory, then what is the basis for believing that the principles operate in the complex and uncontrolled settings of everyday life? Shull et al. (1989) are able to discern the principles in ordinary occurrences of human behavior—if the behavior is viewed at "the appropriate level of abstraction" (p. 71). But they also contend that even within the confines of the "human Skinner Box," the determinants of human behavior are "simply too complex for straightforward relations to emerge" (p. 72). We find it hard to reconcile these disparate views. At the least, Shull et al.'s view of the relation of the laboratory to the world of everyday life appears opposite to that of the other natural sciences. In physics, for example, Newton's laws of motion are not apparent in everyday experience—ordinarily, we do not see objects travel in straight lines for indefinite periods at constant velocities. But the laws can be demonstrated under laboratory conditions that control countervailing forces arising in wind resistance and the like.

Shull et al. (1989), Dinsmoor (1983), and many other behavior analysts are confident that the conditioning principles developed in the animal laboratory do operate in the world of human affairs. Let us underscore that we count ourselves among them. Unfortunately, our faith is not shared by critics of behavior analysis. Until recently, their objections concentrated on the questionable validity of the other types of evidence used to support an operant analysis—that is, evidence from the realms of applied behavior analysis and behavioristic interpretation. These objections now are buttressed by references to the confusing array of findings that has emerged from the human operant conditioning laboratory (e.g., Bandura, 1977; Lowe, 1983; Wearden, 1988). The inability to validate animal-based principles through direct experimental tests with humans can only hinder the building of bridges between behavior analysis and other disciplines such as economics, decision theory, political science, and education—disciplines that behavior analysts believe they should be able to inform and influence (cf. Miller, 1983; Rachlin, 1989; Skinner, 1968, 1975, 1978).

The necessary consequence of devaluing laboratory studies of human conditioning is to increase the burden placed on evidence derived from application and interpretation. But it is in the nature of this other evidence that it cannot provide unequivocal support for the generality of the animal-based principles. Concerning the relevance of applied findings, various writers (e.g., Baer, Wolf, & Risley, 1968; Estes, 1972) have pointed to the inherent conflict between the therapeutic remedial goals of applied research and the need in basic research to manipulate variables freely in line with theoretical considerations. Estes (1972) put his concerns this way:

> Up to a point there can be no doubt regarding the practical efficacy of the techniques of reinforcement.... But as extrapolation continues, questions and doubts begin to arise. Does the effectiveness of reinforcement procedures, however impressive within a limited sphere of application, unequivocally attest that we understand the processes involved? (p. 723)

As an experimental psychologist, Estes sought more direct evidence for conditioning principles in laboratory studies with humans. His efforts, however, increased his skepticism about links between the activities of the clinic and animal research, and he concluded that it is "unrealistic to expect that the effective management of human behavior will be much advanced by direct application of the techniques of the animal conditioning laboratory" (p. 729). These doubts about the applied area as a proving ground for operant principles were echoed a few years later in this very journal, when Pierce and Epling (1980) observed that much of what is done in the name of "applied behavior analysis" is, in fact, out of touch with basic principles from the laboratory. (For a range of opinion on this issue, most of it in line with Pierce and Epling, see Baer, 1981; Deitz, 1978, 1982, 1983; Epling & Pierce, 1983; Hayes, Rincover, & Solnick, 1980; Michael, 1980; Poling, Picker, Grossett, Hall-Johnson, & Holbrook, 1981; Schwartz & Lacey, 1988; Woods, 1980.)

Interpretations also seem to have had limited impact, except to generate more interpretations and, perhaps, to evoke a sense of self-satisfaction with the apparent scope of the explanatory principle. Consider a recent citation analysis designed to measure the influence of Skinner's (1957) operant interpretation of verbal behavior (McPherson, Bonem, Green, & Osborne, 1984). Although Skinner's book had received considerable attention in the years covered by the study (1957–1983), only about two percent of the citations were found in empirical studies in the areas of basic and applied behavior analysis. Interpretations of less complex matters have not fared well either. Attempts to identify the operation of reinforcement schedules in common environments such as schools and factories have been widely criticized—by behavior analysts themselves—as simplistic and superficial (e.g., Crossman, 1983; Michael, 1980; Poppen, 1982).

Thus, although application and interpretation may have their uses in the experimental analysis of behavior, they cannot establish the viability of an operant account of human behavior, at least not by themselves. Also needed is compelling evidence that the basic principles, heretofore discovered with animals, really do operate in human behavior. We see no alternative to seeking such evidence in the laboratory. And although we encourage innovative laboratory approaches, we see no reason to discount the role of the "human Skinner Box" in this effort. Indeed, its value as "an analytic preparation" is strictly dependent on the extent to which the traditional methods of the experimental analysis of behavior are properly applied. We have placed special emphasis on three aspects of the methods: first, that experimental variables are imposed long enough to manifest their effects; second, that behavior is studied as a steady state; and third, that subjects are matched in terms of factors that cannot easily be brought under experimental control. When these precepts are violated, it is the application of the methods that should be questioned rather than the methods themselves.

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